others, have not, however, followed Dana—a crystal right-hand crystallographically, is also right-hand optically with them. F. CHESHIRE.

Imperial College of Science,
South Kensington,
October 27.

I AM much indebted to Prof. Cheshire for stating so clearly the historic incidence of the confusion which has arisen in regard to the designation of the two types of optical rotation, as to which shall be called right-handed and which left-handed, due largely to the reversal of Biot's convention by Sir John Herschel, and to the similar reversion in the second edition of Dr. Pereira's book. Other investigators and experimenters have also adopted the reversal, for instance, Sir William Spottiswoode at the time he was president of the Royal Society, for on pp. 47-48 of his book, "Polarisation of Light," we read: "A right-handed ray is one in which, to a person looking in the direction in which the light is moving, the plane of vibration appears turned in the same sense as the hands of a watch." Moreover, if instead of using the polariscope as a table instrument one projects the phenomena on the screen, the picture there displayed is reversed exactly like a lantern slide, which has to be inverted in the lantern (the two spots in front at the top being brought to the bottom at the back), in order to get an upright picture on the screen. Thus, for example, in the mica-sector experiment of the late Prof. S. P. Thompson (pp. 1103-1104 of the second edition of my "Crystallography and Practical Crystal Measurement"), the black cross moves on the screen one sector to the left for a right-handed quartz crystal and to the right for a left-handed one; whereas on looking through the same instrument used as a table polariscope the movement is to the right for a right-handed crystal, in accordance with the Biot convention.

It is thus important to know the exact conditions of the experiment whenever the question of the correct discrimination of right- or left-handedness in the optical rotation of crystals is being dealt with. Further, the safest course, in the case of quartz, is to cut the section-plate to be used to afford the definite decision from a crystal which is clearly a single individual, and not a twin, showing the little *s* and *x* faces unmistakably, and this course was pursued by me in the preparation of my "Crystallography." As most in accordance with current practice (that of von Groth and Pockels, for example), and in rightful deference to Biot, the discoverer of the two optically active kinds of quartz, Biot's convention was used, in both editions of the book, a course which it is satisfactory to learn meets with the approval of Prof. Cheshire. The apparent opposite, on p. 1101, lines 7-8, is due to this being a projection experiment, the observer looking towards the screen along with the light rays; the direction here, however, really does not matter, as only the colour of the centre of the field is being referred to; even here, perhaps, it would be better in any future edition (they were not present in the first edition) to omit the words "from the point of view of the observer looking in the same direction as the light is being propagated," the text then conforming clearly with the Biot convention. On p. 1083, where a table experiment is being referred to, and the conditions are otherwise similar to those in the centre of the field in the case just referred to, there is no ambiguity, the Biot convention being clearly followed.

It was felt desirable to direct attention to the confusion which has so obviously arisen, and this was done in the footnote to p. 1082. Emphasis was attempted to be laid on the fact that, after all, the phenomena are due to the passage of the ray through the helical structure of the crystal, now so happily confirmed by Sir William Bragg's X-ray analysis of quartz, and that the observer does well to imagine himself travelling with the rays through the screw, in order to appreciate the cause of the rotation of the plane of polarisation or vibration of the light rays. The particular screw type, right- or left-handed, is the same, however, whether we regard the screw from one end of it or the other, whether we look along with or against the light stream; otherwise it would matter which side up the quartz plate were arranged, that is, which side were placed the nearer to any particular one of the nicols. But the optical effect, the rotation of the plane of polarisation or of vibration of the light rays, is, of course, what matters and what is so clearly different for the two different types of quartz helices, and it was my intention to retain and use the Biot convention for its directional (right- or left-handed) designation. The footnote in question is not sufficiently explicit, and must be amended in any future edition. Prof. Cheshire and the writer are, however, quite agreed on the facts, and that this Biot convention shall be the one employed, and I am grateful to Prof. Cheshire and to the editor of NATURE for affording me the opportunity of stating this. A. E. H. TUTTON.

Space-Time Geodesics.

IN his letter in NATURE of November 25, replying to mine which appeared in NATURE of October 28, Prof. Piaggio points out that the equations of Space-Time geodesics may be deduced by other methods than those of the calculus of variations, and suggests that, in some such way, it is possible to get over the difficulties to which I directed attention.

My criticism, however, was directed, not merely against the definition of Space-Time geodesics as *minimum lines*, but against all seeming definitions of them which start from ideas of measurement as a fundamental basis.

I must, however, in passing, warn my readers against what at first sight looks like a suggestion, though I have no doubt that it was not so intended by Prof. Piaggio, that Space-Time geodesics might be defined in terms of "*the osculating plane*."

If there were any strict analogy with the case of geodesics on surfaces in ordinary three-dimensional geometry, such an "*osculating plane*" would (apart from a line of intersection) have to lie in some mysterious region outside our Space-Time continuum altogether. Prof. Piaggio, however, I have no doubt, wishes to lay stress upon the equations he obtains.

I was of course aware that the *equations* of Space-Time geodesics could be arrived at by various analytical devices; but how much better off does this leave us? Consider, for example, the simple Space-Time analysis as given by Minkowski and see what it implies.

In the first place, it implies a set of co-ordinate axes *x*, *y*, *z*, and *t*, which are themselves *geodesics*.

How are these particular geodesics to be defined?

They cannot be defined as minimum lines, for they are not minimum lines; and we cannot use our co-ordinate system to define them, since we are now contemplating how the co-ordinate system can be set up.

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In the second place, the co-ordinate axes are supposed to be *normal* to one another. How is this *normality* to be defined? It is to be remembered that *normality* in Space-Time theory is a wider conception than normality in ordinary geometry; since in the former we have lines which are "*self-normal*" (or what I have called "optical lines"), in addition to other rather curious features.

In the third place, the co-ordinate axes are supposed (if I may so express it), to be capable of graduation in equal parts. How is this graduation to be theoretically arrived at, and how are we to compare lengths, say along the axis of x, with lengths in some other direction?

It thus appears that we are reasoning in a circle if we attempt to give an analytical definition of Space-Time geodesics on such a basis.

The employment of generalised co-ordinates does not free us from difficulties, for, if it gets us out of one difficulty, it brings in another in its place.

Thus, for example, if the "graduations" were made according to arbitrary scales, the expression for the length of a Space-Time interval would contain functions the form of which would depend upon the arbitrary character of the scales employed.[1]

Again, if measurement of intervals be regarded as a fundamental conception, what is one to make of a case where

$$(x_1-x_2)^2+(y_1-y_2)^2+(z_1-z_2)^2-(t_1-t_2)^2=0,$$

in which (x_1, y_1, z_1, t_1) and (x_2, y_2, z_2, t_2) are the co-ordinates of two Space-Time points?

It must not be supposed that in raising these objections to the ordinary methods of treating this subject I am concerned only with destructive criticism.

My own answers to these difficulties are to be found in my published work.

In conclusion, I must thank Mr. Rogers for his very interesting letter published in NATURE of November 25; which, however, does not call for any special reply.

ALFRED A. ROBB.

Cambridge, November 27.

A New Type of Electrical Condenser.

SINCE an electrical condenser is a device for storing electricity, it follows that a secondary battery is a condenser—obviously of very large capacity as compared with the electrostatic type of condenser. Arguing from this point of view it appeared to the writer that, by a suitable arrangement of pasted lead grids immersed in dilute sulphuric acid and connected in circuit with an alternating current, it should be possible to obtain the characteristic effect of an electrical condenser, namely, a phase advance of the current relatively to the terminal potential difference.

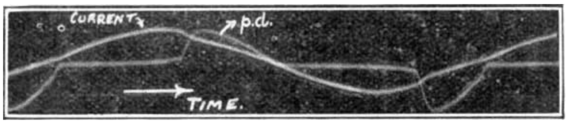

FIG. 1.

A large number of tests have borne out this conclusion and it may be of interest to readers of NATURE to show an oscillogram of the effect (Fig. 1). The oscillogram was taken during a test on a cell consisting of grids pasted with red lead and immersed in dilute sulphuric acid. The temperature of the electrolyte was about 86° C., the frequency of supply was 3·5 cycles per second, the current density about 0·75 amperes per square inch of grid and the r.m.s. value of the terminal pressure was a little more than 2 volts. There was practically no gassing of the cell during the test.

If the fundamental of the potential wave is determined it will be seen that there is a large angle of phase advance of the current on the pressure. There is one other interesting point noticeable in the oscillogram, namely, the potential difference of the grids remains relatively very small during a large portion of the current wave. When the current wave has passed its maximum value the pressure quickly rises to a maximum and then falls to zero at about the same moment as the current reaches its zero value. The process is then repeated during the next half of the current wave.

T. F. WALL.

Edgar Allen Research Laboratory,
The University, Sheffield, November 25.

[AN engineering contributor to whom we have shown Dr. Walls' interesting letter writes: "It has been well known to electricians for the last thirty years that an electrolyte with metal plates in it will act as a condenser. These devices are called electrolytic condensers and are used in everyday work. They generally consist of aluminium plates immersed in an electrolyte, but iron plates in a solution of soda are sometimes used. They are useful for getting currents which lead in phase the supply voltage. Dr. Gunther Schulze carried out an extensive series of tests on electrolytic condensers at the Reichsanstalt in 1909. See *Elektrotechnik und Maschinenbau*, 'Kondensatoren Grober Kapazität' (vol. xxvii. p. 247, 1909)."—EDITOR, NATURE.]

Sex of Irish Yew Trees.

AFTER extensive inquiry up and down the country, I have so far failed to come across any example of the Irish Yew bearing male flowers. All the trees examined in private gardens and in cemeteries and churchyards have been of the berry-bearing or female sex.

I have now a number of young plants raised from the berries of the Irish Yew (*Taxus fastigiata*) fertilised by pollen from the English variety (*Taxus baccata*). These show a graded series from the spreading English type to the erect Irish form.

Growth is so slow, however, that it will be some years before it will be possible to ascertain the sex of these plants; meanwhile, I should be glad to know through the readers of NATURE any case of a male Irish Yew.

If, as is believed, the Irish Yew trees now growing in England have all been propagated by cuttings from the mutational Irish form, which first appeared in Co. Fermanagh, Ireland, more than a hundred years ago, this would explain the fact that they are all of the female sex. On the other hand, it is desirable to ascertain whether any linkage originally existed between erect habit of growth and female-ness in the mutational Irish variety.

Further, if any male example of the Irish Yew can be discovered it would be desirable to test the effect of fertilising the female Irish Yew by Irish pollen.

C. J. BOND.

Fernshaw, Springfield Road, Leicester,
November 28.

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[1] This is the same sort of point which would arise, for instance, in thermodynamical theory by using some arbitrary scale of temperature nstead of the thermodynamic scale.

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